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The agitation of thought is the beginning of Truth.

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ON THE OBSERVATIONS OF NATURE IN THE TREATMENT OF DISEASE.

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[*Concluded from page* 116].

It is true that as yet we know so little of the course and tendencies of many forms of disease, that we might often be at a loss what treatment to adopt; but the clear recognition of our ignorance is the first step towards the acquisition of knowledge, and more enlightened observation would gradually remove the obscurity which at present prevails.

If any of these remarks should be considered by some to imply a want of faith in professional aid, I have only to reply that no conclusion can be more unfounded. With all its imperfections on its head, medicine, in the hands of discriminating and experienced men, seems to me already to be the source of the greatest benefits to suffering humanity. When cultivated with more constant reference to sound principles, it will become still more beneficial in its applications. It is a deep conviction of this truth which makes me so desirous to assist in the good work of medical improvement to which you are now devoting yourself.

It would be easy for me, were it needful, to point out numerous instances in practice, in which medicines were prescribed without reference to any guiding principle, or natural tendency in the system at the time, and in which, consequently, results ensued which were wholly unexpected. In this respect I cannot exempt myself from the censure I have bestowed upon other practitioners; and I must further admit that, even after I became fully alive to the importance of endeavouring on all occasions to act as the assistant and interpreter of nature in the treatment of disease, I continued to meet with many cases in which I could not discover what the real order of nature was, and in which, consequently, I was obliged to resort to purely empirical treatment, and with necessarily varying success. But even then I had this advantage on my side, that the abiding consciousness under which I lived of nature's presence and power inspired me with watchfulness for the observation of her earliest indications, and induced me in the meantime to borrow all the aid I could from her, by placing the various bodily functions, as far as possible, under the conditions most favourable for their healthy operation. When doing so, I have sometimes been rewarded by the gradual disappearance of difficulties which seemed at first irremediable, and by an amount of improvement which served to increase my faith in the restorative powers of nature even under unfavourable circumstances.

The one great principle, then, to which a comprehensive review of Homœopathy, "Allopathy," Hydropathy, and all other systems of medicine, seems irresistibly to lead is, that, in all cases and on all occasions, *nature is truly the agent in the cure of disease; and that, as she acts in accordance with fixed and invariable laws, the aim of the physician ought always to be to facilitate her efforts, by acting in harmony with, and not in opposition to, those laws.* Disease, as already remarked, is a mode of action of a living organism, and not an entity apart from it. In accordance with this view, experience shows that when we favour the return to a normal action by simply natural means, recovery will ensue in most cases, without the use of drugs at all. So far from being always necessary to a cure, drugs are required only where the power of nature to resume her normal action proves inadequate or is impeded by a removable obstruction. Even then, it is still nature acting in accordance with her own laws that brings about the cure. She may be *aided*, but *she ought never to be thwarted;* and medicine will advance towards the certainty of other sciences only in proportion as we become saturated with this guiding principle.

A few words now on Homœopathy in particular. I am very glad that you have brought the question of its truth and merits seriously before your readers; for, of all methods of advancing the interests of science, that which consists in the supercilious neglect of alleged new discoveries, merely on the ground that they differ from what *is* already known, is assuredly the worst. We

know far too little of the constitution of nature, and more especially of animated nature, to be able to decide *a priori* what can or cannot be true regarding the mode in which vital operations are conducted, or in which they may be modified by external influences. Medicine itself is in its very essence an estimative science, and the truth of the principles on which it rests can be ascertained or verified only by careful and extensive observation. Theoretically, these principles may be rendered more or less probable in the eye of reason, but they never can be demonstrated except by an appeal to experience. Medicine, moreover, considered as a system or body of doctrine, is still at the best in a very defective state. Every page of your Review admits and laments this unfortunate truth. We ought, therefore, to extend the hand of welcome to every man who is able either to correct an established error, or add a new truth to the existing store; and much more so, if the offered contribution should be that of a new and important principle capable, if true, of modifying and improving the whole field of medical practice. Not that we are by any means called upon to run after and examine every new theory or alleged discovery in medicine, merely because it is announced to be such. If we did, we should impose upon ourselves a never-ending and most useless task. But surely we are bound not to be too rash in rejecting, without examination, facts and principles which come before us, attested by men of experience, skill, and integrity, and who can have no motive for deceiving us. Judged of by the standard of our own opinions, these facts and principles may seem at first sight to be altogether absurd; but, if so, the question then comes to be, is our standard itself undoubtedly a correct one? Or may it not be that ignorance has misled us to adopt it as infallible, and that it would be wiser in us to compare both it and the alleged discoveries with nature before assuming either to be demonstrably true? Had this reasonable course been followed with the discoveries of Harvey, Jenner and Gall, how much idle and acrimonious disputation and professional obloquy might have been avoided, and how many benefits might have been obtained which were lost for years to suffering humanity, by the opposite course of first rejecting and ridiculing, and then examining evidence only when compelled to do so by a humiliating, because tardy and ungracious, necessity.

Let not this wholesome lesson, then, be lost to us who are the living successors of those who acted so unwisely. To use your own words, Homœopathy, whether true or false, comes before us for examination with "claims on our attention which cannot be gainsayed." It is, you say, an ingenious system, "professing to be based on a most formidable array of facts and experiments," and "woven into a complete code of doctrine with singular dexterity and much apparent fairness." Its discoverer and chief cultivators are, as you believe, "sincere, honest, and learned men." Dr. Fleischmann, perhaps the most eminent among them, is considered by you as "a regular, well-educated physician, as capable of forming a true diagnosis as other practitioners," and as "a man of honour and respectability," whose testimony as to matters of fact "you cannot therefore refuse to admit." Even in acute diseases, the results of his treatment are such as "would have been considered as satisfactory by any candid physician;" and, according to you, even his own narrative affords ample evidence that many of his cases *were* severe; and you candidly add that this was confirmed to you by the private testimony of a competent physician who followed his practice for three months, and himself traced the progress of the pneumonic cases by careful auscultation through all their stages up to perfect recovery, which took place in as short a time as under the most energetic treatment. In some eruptive and febrile diseases, the mortality is stated by you to have been below the ordinary rate. Let us scout quacks and pretenders as we may, here is surely too strong a *prima facie* case to warrant our dismissing it with mere ridicule and contempt, and one which amply justifies you in the course you have adopted of seriously investigating its claims. I am aware that you have been blamed by many for occupying your pages with even a refutation of "such trash;" but so far from participating in this feeling, my chief objection to your review is that it does not go far enough to be *conclusive*, either for or against Homœopathy. You have admitted too much, and denied too much, to warrant your either pronouncing a definitive sentence or reposing in *mere opinion* against its truth. Had you shown that the general results of its practice were *less* favourable than those of ordinary medicine, you might legitimately have held yourself absolved from going further; but in your present position you can no longer stand still. If, as you admit, the truth of Homœopathy is a *question of fact and experience*, which no mere argument can set aside, you are bound in reason and in logic *to test its facts* for yourself before pronouncing authoritatively that it is not true, and more especially before stigmatising it as "useless to the sufferer and degrading to the physician." However improbable its doctrines and practice may be in an *a priori* point of view, it is not by argument or ridicule that its alleged stronghold of facts can be successfully assailed. *As a matter of theory*, supported only by argument, Homœopathy produces no conviction whatever on my mind of its truth, or even of its probability:

but as *a question of fact*, claiming to rest "on the irresistible ground of its superior power of curing diseases and preserving human life," and on the alleged experience of able and honest men, as competent to judge as most of those who oppose them, I cannot venture to denounce it as untrue, because I have no experience bearing especially upon it to bring forward, and we are still too ignorant to be able to predicate *a priori* what may or may not be true in the great field of nature. But after the presumptive evidence which you yourself have produced, if I were now in practice I should hold myself bound, without further delay, to test its truth by careful and extensive experiments; because, where truth is really our aim, the shortest and least encumbered approach to it is always the best; and even a few well-defined and carefully observed facts would carry far more weight, as *items* of evidence, than volumes of general or controversial reasoning. In instituting such an inquiry, however, we ought to be prepared to lay aside prejudice, and to scrutinise facts with the fairness and liberality characteristic of a love of truth, and not regard them with dislike and distrust, as if they were as many live embers purposely laid down to burn our fingers the moment we touched them. View the question as we may, *one of three things must be:* either Homœopathy is true, or it is false, or it is a mixture of truth and error. Let us suppose the worst, and hold it to be false in its foundation, and false in its superstructure, what harm can result from putting it to the test, and ascertaining the fact demonstrably? None whatever, but, on the contrary, much good. We shall at least *have gained the power of giving a direct and authoritative negative to its allegations*, which we shall then prove to be fallacious, and which have been suffered to reign and diffuse themselves for thirty years from the absence of *direct* counter-evidence by which to rebut them. We shall thus be able also to put the profession and the public on their guard with some chance of being listened to, and shall have obtained the inestimable advantage of keeping our own minds open to the admission of new truths, and of showing that in our estimate of evidence, and in our conclusions, we are actuated not by any mean jealousy or dogmatic assumption of authority, but by the single and simple desire of advancing the interests of science and humanity to the best of our ability. The *very worst* that can happen in the event of its being wholly untrue is, that we shall have bestowed some time and pains in obtaining the means of more effectually putting down a great error; while, as a compensating advantage of no small value, we shall have at once increased our knowledge and cultivated and strengthened our intellectual and moral faculties, by the very nature of the mental exercise which such a scrutiny requires; and surely these will be rewards well worth all the time and trouble which they may cost us.

If we adopt the supposition that Homœopathy embodies an *admixture of truth and error*, the inducement to institute a rigid and careful inquiry into its claims becomes still more imperative, that we may obtain possession of the one, and carefully avoid the other. The degree of success, be it more or less, which all admit to attend Homœopathic practice, as conducted by such men as Fleischmann, is sufficient to show that either the system or its advocates possess *some* advantages in the treatment of disease, which it would be useful for ordinary practitioners also to examine and adopt. Whether the means which afford these advantages be derived from the domain of hygiène, of materia medica, or even of the imagination, is of comparatively little practical consequence, provided *their utility to the patient and the best mode of reproducing and applying them to the treatment of disease* can be clearly established. This, however, can be done only by careful investigation, and that such investigation would be amply rewarded may fairly be presumed, from the good already effected by Homœopathy in demonstrating the evils attendant on that over-active medication which characterises so much especially of English practice. Ordinary medicine is now not nearly so heroic and undiscriminating in the use of strong measures as it was some years ago, and this improvement is unquestionably due in part to the progress of Homœopathy, as well as to the natural increase of our knowledge.

The remaining, although unlikely, supposition, viz., that Homœopathy shall prove to be *essentially true* in its fundamental principle, and consequently fraught with benefits to science and humanity, as its advocates affirm it to be, need not detain us more than a moment. *If true*, how much more shall we then have reason to rejoice that we did not look upon its claims with prejudiced eyes, or reject and condemn it unheard and unexamined! Had Harvey's detractors examined his facts first, and then given their verdict, how different would the results have been to themselves, to him, and to mankind! And yet in our own day the profession acted towards Jenner, and also towards Gall, as if Harvey's name and memory had been blotted from the page of history.

I press all these considerations upon you, not from any particular leaning towards Homœopathy, or any other new and disputed branch of knowledge, but because of the transcendent importance of cultivating science in a right spirit, and offering truth a ready and unprejudiced welcome from whatever quarter it may come. Ridicule and

declamation may be rightfully employed to explode errors *after they shall have been proved to be so;* but they are most unfit instruments for the primary investigation of truth, and as such ought to be banished for ever from scientific discussion, and a candid spirit of philosophical inquiry be instituted in their room. I have had no personal experience of Homœopathy, and am consequently, as little inclined to admit as to reject its claims, but I should wish to steer clear of prejudices regarding it. There are perhaps a few analogies in its favour, but its doctrinal expositions embody much that is crude and contradictory, and most of its practical evidence, in the shape of published cases, is rendered nugatory by the same sources of doubt which render so much of professional experience and testimony inconclusive, if not worthless. Sufficient discrimination is not used, or if used not recorded, to warrant much reliance on the alleged connection between the remedy and the recovery in individual cases. As in ordinary medicine, the *post hoc* is too universally assumed to imply the *propter hoc*. If I am not mistaken, the more intelligent Homœopathists themselves admit this, and in consequence do not claim *belief* on the ground of the recorded cases, but affirm that, on the contrary, rational belief can be produced only by personal and extensive experience. But, while I refuse belief, I can see no reason for that deadly hostility which many feel towards *the principle* of Homœopathy. If it be true, such hostility is misplaced and injurious. If false, it is needless and supererogatory; for the hostility will vanish with the non-existence or destruction of its object. And, after all, why should either party delight in representing Homœopathy and ordinary medicine as *in every respect* opposed to each other? In a large proportion of cases, the more rational and enlightened men of both parties employ the very same hygienic and general means which we have already seen to act so large a part in effecting recovery; and the chief difference between them relates to the principle on which the requisite medicine is to be selected. The Homœopathist prescribes according to the principle of *similia similibus*, because experience, he says, proves this to be the safest and most efficacious plan. The ordinary practitioner, on the other hand, prescribes that which rational, or it may be routine, experience has led him to believe the best adapted for cases of the kind before him; and without stopping to inquire whether its action is homœopathic, allopathic, or antipathic. Surely there is no necessary cause of quarrel in all this, but merely results to be tested by careful experiment. "True," you may say, "but then the infinitesimal doses are so absurd." They certainly look very absurd; and I at once admit that nothing short of demonstration and personal evidence will ever inspire me with a conviction of their power to do either good or harm. But then all Homœopathists say that it is the principle of *similia similibus*, and not the dose, which constitutes the essential element in their system, and that the infinitesimals may be discarded, and yet the great principle of Homœopathy remain unshaken. This latter, then, is the great fact to be proved or disproved, to settle the question for ever; and why should it not be put to the test? Let experiments be made on a sufficient number of healthy persons with quinine, or any other drug, to ascertain whether it really has the property ascribed to it of exciting certain groups of symptoms in a sound constitution, and, after carefully varying and repeating the experiments, faithfully record and publish the results. Surely there is nothing unphilosophical or undignified in instituting such an inquiry, and nothing so difficult as not to be easily overcome by judgment and patience. Having tried their action in health, try the same remedies *in the usual doses* in the treatment of disease with as much care and discrimination as possible, and again record the results. If the principle holds good, let us adopt it, and be thankful we have now a surer guide than before. If it fails, our exposure of its fallacy will tell with tenfold effect, from being founded on direct experience. In the same way with the infinitesimal doses, let us go at once to facts, and leave mere disputation to the idle speculator. All truth is harmonious, and what is true in the one system must harmonise with and throw light upon what is true in the other, and, consequently, it would be better for science were both parties to endeavour to find out the points of contact rather than those of repulsion. In the very nature of things, certainty or absolute identity of opinion is, and ever must remain, an impossibility; and it ought never to be forgotten that in this respect there is a radical difference between medical and physical science. Physical science is *fixed* and *positive* in its principles and in its details, because its facts are always accessible for examination under the same conditions, or under such variations as can easily be traced and allowed for. Medicine, on the contrary, is and ever must be an *estimative* science, because its facts and phenomena are subject to continual variations from varying states of the body and mind of the patient, which we can neither control nor appreciate with entire accuracy. Its cultivators, too, are men differing in intellectual power, knowledge, skill, and experience; and even, if they were all equal, their judgment is constantly liable to be impaired or disturbed by any slight disturbance of health or excitement of feeling, or even by a little extra fatigue; and hence, although its principles are fixed and determinate, because also founded on the laws of

nature, the soundness of the conclusions deduced from them for the guidance of treatment must ever depend on the soundness of the estimate formed by the physician of their operation and influence in the individual case. Very rarely, indeed, can they be absolute, and hence the wide field for the exercise of sound judgment, skill, and discrimination, on the part of the practitioner, and the mischief which may attend a practice founded on mere routine. Hence the forbearance and charitable construction which, as members of a liberal and useful, but most difficult profession, we are bound to exercise towards each other; and for the exercise of which there is, I fear, ample occasion in this very letter. But restrained as I have been by impaired health, as well as by the impossibility of doing full justice within your limited space to a subject at once so extensive and so important, I could not always express my opinions with the precision which I wished, and therefore I must trust to your good sense and right feeling not to give undue importance to any isolated or dubious expression which you may meet with, but to adopt that meaning which is in accordance with the general spirit of my remarks. My only anxiety has been to help you in the good work to which you have dedicated yourself with so much zeal, energy, and talent, and for which you will, I have no doubt, one day have your reward in a rich harvest of useful results.

I remain, my dear sir, very sincerely yours,
ANDREW COMBE.

A FEW THOUGHTS ON CONSUMPTION.

THE deaths by consumption, as proved by the weekly bills of mortality, are more numerous than by any other disease. The profession seems to act as if every case of consumption were incurable; and consequently the same unsuccessful treatment is pursued, and oftener, much oftener, than is imagined, medication itself becomes the cause of a more fatal rapidity than would take place, if the disease were left undisturbed in its natural course. This opinion was expressed to us recently, by one of the oldest and ablest old school physicians of this city.

It is believed by the profession that *tubercular consumption* is an hereditary complaint. It is not difficult to know those who are likely to be predisposed to this malady. If, on inquiry, there be reasonable grounds to infer such predisposition, then it becomes the duty of the physician to avoid altogether, or if absolutely indicated, to administer with great caution certain drugs which directly tend to awake a slumbering *phthisis pulmonalis*. There are such drugs, and in common use too, which we will try to prove, or present what is so near positive evidence, that few impartial minds can resist the conviction that our views on this subject are true.

Mercury, whether in the form of blue pills or calomel, is, perhaps, the most likely to develope *tubercles*. This mineral has been so extensively and universally used in all kinds of diseases, that its effects in the human system should be better understood than any other drug. The symptoms which follow are mercurial symptoms, ascertained by trials on numerous healthy persons. It will be noticed how strikingly similar the symptoms of *mercury* are to those of cases of *tubercular phthisis*.

The observation of experienced physicians, and the pathogenesis of that mineral should settle the question under notice.

Mercury will cause in healthy persons, when administered in moderate doses, and often repeated: "Great fatigue, weakness, and rapid loss of strength; frequent trembling, even after the least exertion; fainting fits; emaciation and atrophy of the whole body; at night restlessness, agitation and tossing; heat or sweat; palpitation of the heart, short respiration; heat in the face and head, with redness of the cheeks. Copious weakening sweats, day and night, and in the morning; stitches in the side; face pale; ulceration of the gums; entire loss of speech; painful dryness of the throat; inflammatory redness and swelling of the back parts of the mouth and throat; insatiable appetite and craving; great weakness of digestion, with continued hunger, and pressure in the stomach; risings, principally after eating; and often a putrid, or bitter, or sour and rancid taste, violent and empty risings; excessive sensibility in the stomach and in the pericardial region; tension, fulness, and pressure as from a stone in the pit of the stomach, principally during or after a meal, however little may have been eaten; suppression of the catamenia; catamenia too copious, with uneasiness and colic; purulent corrosive leucorrhœa, with itching in the parts; continual hoarseness and loss of voice; dry cough, sometimes fatiguing and shaking, principally in bed, in the evening or at night. Cough excited by tickling, or

a sense of dryness in the chest. Cough aggravated by speaking. Cough as if caused by irritation in the stomach. Cough, with expectoration of pure blood; difficult respiration as from want of breath. Shortness of breath when going up stairs, and when walking quickly. Anxious oppression of the chest and difficulty of respiration. When lying on the left side attacks of suffocation. Lancinations in the chest, principally when coughing. Sharp pains in the shoulders, especially when moving."

Now, if *Mercury* can thus influence the healthy human organism, it is fair to infer, even in the absence of all other proof, that its effects must be pernicious in those who have an undeveloped *tubercular* disease, for that disease, in its natural course in some persons, would present symptoms similar to those of *Mercury*. We do not mean to be understood as saying, that *Mercury* is not admissible even in the treatment of *tubercular phthisis*. When it is homœopathic to a case, it should be administered; but in a high attenuation—the 30th, 50th, or 100th. We have seen, more than once, a single dose of the first trituration of mercury given to a person with developed *tubercles*, cause alarming diminution of strength, and copious sweat in a few hours. It cannot be estimated how extensive the mischief is, that is caused by the ordinary allopathic doses of mercury.

We shall continue this subject and notice the effects of Quinine and the Peruvian Bark on consumptive persons.

PHENOMENA AND THEORY OF INFLAMMATION,

IN CONNECTION WITH HOMŒOPATHIC STATISTICS.

By JOHN OZANNE, M.D.

(*Continued from page* 95.)

IV.

The opinion that the white corpuscles of the blood are increased in inflammation is based chiefly upon the following grounds:

"1st. The accumulation of colourless corpuscles within the vessels of an inflamed part.

"2nd. The large quantity of colourless corpuscles contained in the liquor sanguinis, which, in blood drawn in inflammation, rises to the top to form the buffy coat" (W. Jones, loc. cit.).

With reference to the first point, we have brought forward the authority of Mr. Wharton Jones and of Mr. Paget, to prove that the white corpuscles of the blood are not usually increased in inflamed blood in the human subject, and that, when increased, the increase does not depend upon the inflammatory process. We might add here the authority of Dr. J. Hughes Bennett, who has performed experiments with the view of ascertaining the truth of this part of the theory of Dr. Williams and Mr. Addison. He looks upon it as totally devoid of foundation. "He thinks that the above-named physiologists may have been deceived by the rendering distinct the nuclei of the epidermic cells in the web of the frog's foot; *which nuclei* closely resemble the colourless blood-corpuscles" (Ranking's Abstract, vol. v. p. 185).

Thus far we have found that no augmentation of the white corpuscles of the blood has been proved to exist in the blood *whilst circulating in its vessels*. The case is different when we approach the second point, namely, "the large quantity of colourless corpuscles contained in the liquor sanguinis, which, in blood drawn in inflammation, rises to the top to form the buffy coat."

The increase is distinctly stated by the different observers, and the statement is founded upon the direct observation of the human blood itself. Here, then, is no source of fallacy, such as has been pointed out—no inference from the frog to the human subject.

But the question here presents itself—what is the character of these white corpuscles? are they of the same nature as those observed in the vessels or in blood just drawn from them?

Mr. Gulliver (App. to Gerber, p. 15) states that M. Mandl seems to have been the first to notice the presence of the white globules in the blood of mammalia, and that he considers "that they are produced by the coagulation of the fibrin, and that this coagulation is necessarily attended by the formation of these globules." Hence he calls them *fibrinous globules*. "He moreover states that these globules *are never found in the circulation, but are formed and may be seen to augment in number on the port-object of the microscope;* and Mr. Phillips adopts this view of the subject."

Further on (p. 18) we find the following: "M. Piorry has descried in the buffy coat of the blood, greyish granulations, about as big as poppy or hempseeds, which granulations were best seen by transmitted light (*contre-jour*);" and Mr. Addison has lately given some observations on "Colourless Globules of the Buffy Coat of the Blood," which, it appears, were detected with a common lens. "On dipping the point of the finger," says Mr. Addison, "on the surface (of the buffy coat) before coagulation had taken place, a clear colourless drop adhered to it, which, when transferred to

piece of glass and examined by a common lens, against the light, was found to contain an immense multitude of clear colourless globules." In the next page, Mr. Gulliver says of M. Mandl, that he dissents from the opinion of Professor Muller, that the isolated white globules of the blood of frogs are lymph-globules. M. Mandl regards the white globules "as identical with the fibrinous globules which are formed on the port-object of the microscope, and never seen in the circulating blood."

We believe that these different quotations are already sufficient to raise a doubt whether the authors who have written on the white corpuscles of the blood, have not in general confounded together two sets of globules, different in their appearance and in their mode of origin. It is tolerably evident that the *fibrinous globules* of M. Mandl cannot be the same as the white corpuscles found in the blood by other physiologists. For he states that they are never found in it, but form by coagulation, when on the glass-plate under the microscope. After a careful examination of all the recorded facts bearing upon this question, which we could find, we have come to the conclusion that there are two distinct orders of pale corpuscles to be observed in the human blood (*besides* the very fine granules or molecules described by Mr. Gulliver): first, the white corpuscles circulating with the blood in its vessels; and, secondly, the white corpuscles observed only in blood which has escaped from its vessels in exudations, or which has been artificially drawn from it; most of the latter forming by the process of coagulation which takes place. We also believe that these consist of fibrin.

We have stated our conclusions beforehand, in order that the following observations of M. Andral might be better understood. They are, in our opinion, deserving of close attention, for we deem them to be quite conclusive.

M. Andral found, on examining blood just drawn, that the red corpuscles appeared perfectly regular and their edges well defined; but these soon became granulated (like a raspberry), festooned, and in some cases assumed the appearance of a cogged wheel. At the same time, the white corpuscles which M. Andral had observed in the blood, previously to this change of appearance of the red corpuscles, became rarer as this change progressed and became more conspicuous. He ascribes this phenomenon to the deposition of the white corpuscles (which he has previously described in fresh-drawn blood) upon the red corpuscles.

"In following attentively," says he, "all the movements of displacement which took place at the focus of the instrument, I was enabled to watch the mode of production of this phenomenon, and to see the white corpuscles approach the red globules, deposit upon their surfaces, adhere to their edges, and form thus all the possible varieties of granulated and festooned globules. This granulated aspect, which some have considered as the indication of a commencement of destruction, and others as the result of a pathological influence, is then, after all, only the product of the precipitation of the white corpuscles around the red globules" (Hèmatologie Pathologique, pp. 32, 33).

Now, it must be observed that these phenomena were observed in healthy blood. M. Andral, unlike many other observers, thought it necessary, in his researches upon the blood, to vary his experiments, and to repeat them, under the greatest possible variety of circumstances, *both in health and disease*. Thus, of the microscopical examination of the blood, he says:—"This examination must be made with much care and during a long time for healthy blood, before we can think of studying by means of the microscope the alterations which the liquid may have suffered" in disease (op. cit., p. 30).

Let us now turn to his examination of the blood in inflammation.

"Scarcely had the yellowish and opaline liquid, which constitutes its first rudiments (of the buffy coat), shown itself above the mass of globules, than we placed a drop of it in the focus of the microscope; then, independently of a few red globules, the field of vision of the instrument seemed to us to be full of these granular corpuscles, of which I have previously endeavoured to determine the nature" (op. cit., p. 77).

Now, what is the nature of these white corpuscles? They are found to exist in the buffy coat of inflammatory blood; they are also found in healthy blood; moreover, they are found in the semi-transparent and almost oleaginous stratum which forms on the surface of the blood, constantly and normally in the horse, but only in certain pathological conditions in man.

We thus find that whereas in healthy human blood a number of white corpuscles are to be found soon after it is drawn from the vessels, the surface stratum which is about to form the buffy coat in the blood of the horse in the healthy state, and of man in inflammation, contains a much larger number, for the field of vision is full of them. Now, it has not been proved that white corpuscles are more numerous in the blood of the horse than in that of other animals. These pale corpuscles must, then, be the produce of *coagulation*, as held by M. Mandl.

But the proof of this and of the fibrinous nature of these corpuscles does not rest upon a mere inference alone, however sound it may be. Direct experiments have pronounced in its favor.

"But what is the *nature* of these white corpuscles? Here a general fact presents itself. Whenever the blood, just as it issues

from the blood-vessels, is examined, the existence of isolated white corpuscles is first ascertained, and next the granulated or festooned appearance of a certain number of red globules. When, on the contrary, by any means whatever, *the blood is deprived of all its fibrin before its spontaneous coagulation, no white corpuscles are any longer found in the focus of the microscope, neither do the red globules any longer present either the granular or the festooned aspect*" (Andral, op. cit., p. 33). "*The presence of the fibrin is then necessary*, in order that the drop of blood may contain white corpuscles, and without these the alteration of the red globules cannot take place" (p. 34).

This experiment is decisive as to the *nature of these* white corpuscles. They are fibrinous, and they are produced either soon after the blood is drawn or in the act of its being removed from its vessels, for they have never been seen in it, although *granules* much smaller than are these corpuscles have been seen in the blood, either stagnant or circulating slowly in the blood-vessels.

We have still to state the relation in which these corpuscles stand to those in our former papers. In the first place, they are different from the white or lymph-corpuscles to which Dr. Williams alludes, and to which he ascribes the obstruction in the vessels of inflamed parts. Dr. Williams's white corpuscles, as delineated in his diagram (already cited), are nearly as large, as large or much larger than the flat blood-discs which circulate with them in the vessels. Mr. Gulliver, who has made numerous measurements of the blood-corpuscles in man and many mammalia, as well as in *batrachia* and *invertebrata*, gives the diameter of the white globules as 1-2800th of an inch; the diameter of the red corpuscles being 1-3300th of an inch.

Now, M. Andral has measured the white corpuscles described in his "Hématologie Pathologique," and states their diameter to be 1-500th of a millimeter, the red globules being 1-110th to 1-120th of a millimeter; the diameter of the latter being more than four times that of the white corpuscles, which he has proved to be fibrin, and which accumulated in the buffy coat of inflammatory blood. It stands to reason that, if it were not so, the latter could not give to the former a granulated appearance like that of a *raspberry* or a festooned outline. In fractions of an English inch, M. Andral's white corpuscles are equivalent to about 1-12000th, whereas the large white corpuscles are equivalent to about 1-2800th of an inch. It is clear that, with these data, no confusion whatever can exist with reference to the *respective characteristics* or the *nature* of these two orders of white corpuscles.

It is singular that so late as the year 1846, Dr. Carpenter, in his "Manual of Physiology," should have overlooked these observations of M. Andral. That he had not read M. Andral's observations on the white corpuscles, published three years before, is evident, if we take into consideration what he says of them in his Manual.

"Thus, in the inflammatory process, the quantity of fibrin in the blood is greatly augmented, and the number of white corpuscles found in that fluid, when it is drawn from the body, is very largely increased.... ....Moreover, they are observed to accumulate in great numbers in the vessels of inflamed parts" (Manual of Physiology, p. 125).

It is thus evident that Dr. Carpenter confounds together both kinds of white corpuscles—the large ones, about as large or larger than the red corpuscles, and the small ones, less than one-fourth of the diameter of the red discs. Yet Dr. Carpenter depicts in his frontispiece the very mammillated or granulated appearance of the red corpuscles, which led M. Andral, in his attempt to explain them, and in his researches for that purpose, to such fruitful results.

We now find—these important points gained—first, the large white corpuscles are not usually increased in inflammation, and, if so, only in peculiar conditions; secondly, the number of minute white corpuscles is probably increased; thirdly, the latter, being formed of fibrin, *must* be increased, for the fibrin is known to be greatly increased in quantity in the blood in inflammation; and, lastly, we discover how it is that many pathologists have been led to suppose that the large white corpuscles are augmented in the blood in inflammatory disease.

Let us now sum up all that has been said with reference to the proportion of colourless corpuscles in the blood in inflammation.

I. There are two kinds of white corpuscles to be found in the blood, both in health and during the presence of inflammation:—

A. Large corpuscles, as large or larger than the red corpuscles. These are observed in the blood-vessels, as well as in the blood drawn from them.

B. Small corpuscles, the diameter of which is only one-fourth or one-fifth part of that of the red corpuscles. These are only observed in blood effused from its vessels into the surrounding tissues, or in blood drawn from them. They seem more especially to accumulate on the surface of the buffy coat during its coagulation in the healthy condition in the horse, and in the inflammatory condition in man.

II. The larger white corpuscles do not appear to be augmented in the blood at large, or in the blood-vessels of inflamed parts, in inflammations occurring in persons in an ordinary state of health; but they are found to be augmented in inflammations

occurring in weak and cachectic individuals.

III. The larger white corpuscles are not present in larger numbers in the blood-vessels of inflamed parts than in the vessels of healthy parts. Neither do they seem to be in any way the cause of obstruction in the vessels of inflamed parts.

IV. The smaller white corpuscles appear to be of a fibrinous nature, and to owe their origin to the coagulation of the fibrin of the blood.

V. They are found in healthy blood, but in larger quantities on the surface of the buffy coat during its coagulation.

VI. Since the proportion of fibrin has been proved to be much greater in the blood drawn during the existence of the inflammatory process than in that drawn in health, it is probable that the smaller white corpuscles are more numerous in the blood in inflammation than in health.

VII. But as these corpuscles have never been observed in the blood-vessels either in the healthy state or during diseases, they cannot produce any obstruction in them.

Let us now apply these data to the *treatment* of inflammation, and more especially to the question—Is blood-letting necessary in inflammatory disease?

Since the larger white corpuscles are neither so generally increased in inflammation as to render that increase a constituent part of the inflammatory process, and since it is found that they do not cause any obstruction of the vessels of inflamed parts, or add to it when it exists, it follows that *the removal of a portion of these corpuscles is not necessary.* Hence *there is no circumstance in the physical phenomena of inflammation requiring the abstraction of blood, or pointing to it as a desirable remedial measure.*

So far from the presence of the larger white corpuscles requiring the abstraction of blood, whatever may be their quantity, the observation of Bennett, "that the proportion of the colourless corpuscles is increased by abstraction of blood, and that the portions first drawn in inflammation contain very few," would, if the increase of the colourless corpuscles were still held to be the cause of obstruction of the vessels of inflamed parts, tend to show *that blood-letting can only be injurious in inflammation.*

But we do not wish at present to bring forward the latter conclusion. We are satisfied with having demonstrated that *there is no circumstance* in the physical phenomena of inflammation, which *tends in the least to show that blood-letting is either necessary or likely to be beneficial in the treatment of inflammatory diseases.*

Since the smaller white corpuscles are found to consist of fibrin, we shall say nothing of this fact with reference to the treatment of inflammation, at present; for in our next paper we shall have to consider the alterations in *kind* and *quantity* of the constituents of the blood observed in inflammation; the principal of which is the marked augmentation of the proportion of fibrin.

(*To be continued.*)

ESTABLISHED RULES IN MEDICINE SHOULD BE STRICTLY FOLLOWED IN THE PRACTICE.

It is admitted, and deeply regretted by those competent to judge, that the healing art has not progressed like other branches of knowledge. For many hundreds of years it was virtually the same, with similar unsatisfactory results, as the bills of mortality indicated. At the moment when the ablest of the profession, here and there, ventured more than to insinuate their want of confidence in medicine, and some of these declared they had found better results, by doing little more than insisting on a strict observance of dietetical rules: at this period, Hahnemann, whose observations had led him to look upon allopathic practice as uncertain and unsafe, promulgated principles in medicine, and defended them by the most convincing proofs and arguments. Since then, these principles, which constitute the science of medicine, have been spreading, until the real or pretended disciples of that great man may be found in almost every city and large town in every country. The Homœopathic system is in itself nature's laws, which are clearly defined, and constitute the basis of a true healing art. Allopathy has no such laws; she pretends to rely on experience, in which, if her practitioners would be accurate in their observations, ample proof would be furnished to each investigator, of its pernicious tendency.

The people are not fully awake to this subject; and many who are influenced by allopathic teaching, think that *that* homœopathist who is not strict in his adherence to the rules of the art, shows a liberality consistent with the times; that he wisely avoids exclusiveness and the fanatical doctrine *ultraism;* and further, that such a loose kind of practice shows superior intelligence and judgment. Such sentiments are entertained by many, who of course have never for a moment questioned the soundness of their views. "I like that physician,"

said one, "who studies the good of his patients, and keeps himself out of a straight-jacket; who disregards rules of practice as occasion requires." This doctrine originated in quackery, and is perpetuated by those who do not seem to have examined the subject. We doubt if a physician can be found who does not, so far at least as intention is concerned, consult the good of his patients. The good of the sick can be consulted by the physician only by a strict observance of established rules of practice. Whatever pretense to that object there may be out of such rules, is a deception in the practitioner and also towards the sick.

A CASE OF NEURALGIA.

It sometimes requires a good deal of labor to select the remedies for individual cases of disease. The therapeutic law, and the pathogenesis of drugs seem plain enough in the abstract, but when used in practice, they are often attended with a good deal of perplexity. We were recently consulted by a young lady for a *neuralgia* of the left side of the head, face and neck, with which she had been afflicted nightly for nearly three weeks, and had employed various means for relief, but without effect. The pain came on daily at six o'clock P. M., and continued until four o'clock the following morning. This had been uniform, and the pain was very severe. We searched for the remedy for hours; and after examining all the more prominent drugs, concluded that *Belladonna* or *Nux Vomica* was indicated. At length we almost forced ourself into the belief that the former was the remedy, and gave it for two days without the slightest mitigation of the sufferings. We then resolved to examine the pathogenesis of other drugs not in such general use. And among these, *Guaiacum officinale*, almost word for word corresponded to the case as we had previously recorded its symptoms. Having at the time the 200th attenuation only, we gave three doses, at intervals of two hours while the pain prevailed, which caused a slight aggravation and a change in the hour of the attack to seven o'clock P. M., and terminating at one o'clock A. M. The *Guaiacum* 30th was given the next night every hour, but the aggravation caused the sufferings to be intolerable. The pain ceased about three o'clock A. M. The following day and night we gave nothing, but the pain came on at 8 o'clock, P. M., and continued less violent until about one o'clock A. M. The next day we gave three doses of the same medicine, 30th attenuation, one every two hours between the paroxysms. The result was, no return of the pain; in a word, the patient was cured. *Guaiacum* has these symptoms, which were distinctly marked in the above case: "External headache, as if there were too much blood in the integuments of the head, and as if the head were swollen, when sitting. Pulsative throbbing in the outer parts of the head, with stitches in the temples, removed for a short while by external pressure, relieved by walking, increased by sitting or standing. Lancinations in the right cheek, as if knives were plunged in. Pain in the left side of the jaw." These symptoms, with others similar in character on the neck, which are not found in *Guaiacum*, were described with unusual accuracy by the patient, except that the sufferings were on the left side of the head, face and neck, bounded as if by a line drawn from the angle of the jaw, before the ear to the vertex; from thence to the tip of the nose and left nostril to the angle of the mouth; from thence over the jaw to the neck, affecting about one third of it.

We are thus particular, that the student in Homœopathy may have an example, how clearly the pathogenesis of drugs does sometimes correspond to the symptoms of disease; and also how prompt the curative effects when the medicine and the disease are brought within the therapeutic law. The symptoms of *Guaiacum* as above, it is recorded, took place on the right side of the head and face, which this case shows was of no importance when the character of the symptoms is so very like those of the disease it no doubt cured.

The Annual Meeting of the Homœopathic Medical Society of the State of New-York will be held at the City Hall in the City of Albany, on Tuesday, the 8th day of February, 1853, at 10 o'clock A. M.

HENRY D. PAINE, M.D.,
Secretary.

THE AGGRAVATION OF DISEASES BY THEIR REMEDIES SHOULD BE RECOGNISED.

We have often urged the importance of accuracy in the homœopathic treatment of diseases; in recording the symptoms; in selecting the remedy; in its preparation; in the size and repetition of the dose; and what is, we were about to write, of the greatest importance, to learn to recognise the re-action of the vital power from the impression made on it by the drug administered. This requires a close, long and patient scrutiny. If we could command the language, we would make an impression on the mind of the reader strong and lasting, that to recognise the effect of a remedy after its administration, however slight that effect might be, is an *essential* duty of the homœopathic practitioner. It requires, we know, a most critical examination; and daily and hourly practice will make it more and more easy. It puzzles us how a practitioner knows when to cease repeating the dose of the remedy, if he overlooks the re-action caused by it; if this re-action is denied—for there are those who do deny it—then it follows that drugs cause no effects whatever. We will not, however, argue the point at this time, but assume what is, we think, proved—that drugs, attenuated drugs, even to the 30th, 50th, 100th, and higher, do cause a disturbance of the vital power, each developing characteristic symptoms, which can and should be recognised by the practitioner. These effects can be perceived and reduced to a certainty only by a most careful and thorough comparison of the pathogenesis of the drug employed, and the symptoms of the disease, as the latter existed previously to the administration of the remedy, and afterwards. If this examination is conducted as it should be, it may be noticed, that in some cases, all or nearly so, of the disease's symptoms will be aggravated in a greater or less degree. In other cases a few of them only will become more violent; and if the remedy has been taken in too large a dose or too often repeated at short intervals, then some of the characteristic symptoms of the drug may be developed; and this takes place, too, even when the remedy is highly attenuated, and not unfrequently from a single dose. The vast importance of this knowledge, in every case of disease, to the homœopathic practitioner, can be estimated only by those who habitually and perseveringly engage in such critical examinations of their cases.

It is a habit of some to ascribe, without investigation or thought, an exaltation of the symptoms to the natural course of the disease, which in some cases is true, but not by any means in all; and because of these facts, how important it is to be able to discriminate in the way we have stated. To show the value, the practical value; in fact, to show how indispensable is the knowledge to which reference is here made, we mention an established fact, which is this: that when the vital force responds to the impression of a remedy, by what is termed a re-action, if the dose be repeated, its curative effect which has begun to be developed, will be disturbed, and so much so in some cases, both in acute and chronic diseases, especially in the latter, as to be defeated; and the drug which was acting as the remedy cannot again be employed in the case, even while the symptoms seem to indicate it. In connection we will mention another fact; that this is the reason why practitioners go on administering from day to day, and several times in each day, the same drug which the symptoms more and more clearly indicate almost after every dose, and yet a cure does not follow; and it may be, we remark further, that it is under such conditions that practitioners spring to the conclusion that although the remedy is distinctly perceived by them in accordance with the therapeutic law, yet both the law and the drug are at fault on account of some unknown peculiarity in the case. And therefore they resort to really unknown allopathic measures. The mismanagement of remedies will defeat cures; and unless practitioners of our school direct their attention to the subject of this brief article, they may rely on it, their success in the cure of human maladies will not be such as the science of Homœopathy fairly warrants anticipation.

It is not unusual for a few prominent allopathists of this city to say to their patients who are inclined to favor Homœopathy, that they treat them according to that system. The New-York Academy of Medicine should investigate this subject. If it wants names and proof, apply to the editor of this Journal.

ACCOUCHEMENT OF MARIE ANTOINETTE.

Marie Thérèse Charlotte, *fille du roi*, or, as she was afterwards called, Madame Royale, was born at Versailles on the 19th December, 1789, and her entrance into a world, which was to be to her a scene of sorrow and mourning, had nearly cost the life of her mother. The severe sufferings of the queen, which necessitated extreme, measures, were augmented by the barbarous custom then prevalent at the court of France, of admitting the public to witness the birth of an heir to the crown. Madame Campan has given a graphic account of the scene that passed in the chamber of Marie Antoinette at the moment of her accouchement, and the fearful risk to which she was exposed by such an abuse of the privilege of the people. The royal family, the princes of the blood, and the ministers of the crown had passed the preceding night in the rooms adjoining the queen's bedchamber, in hourly expectation of the event; but, at the approach of the critical moment—a moment when in all cases the greatest precaution, the utmost tranquillity, are necessary to ensure the safety of the sufferer—the public were admitted, who burst tumultously into the room, and filling it to suffocation, not only impeded the movements of those whose assistance was required on the august patient, but heated the atmosphere to a degree that produced the most alarming results. Had not the king's forethought caused the tapestry screens which surrounded the queen's bed to be secured by strong cords, they would inevitably have been thrown down upon her by the rush of people, some of whom mounted upon the consoles and cabinets in order to obtain a more commodious view of the royal sufferer. The noise made by them—the stifling heat they occasioned—perhaps, too, the disappointment experienced by the queen on finding that her child was not a son, produced one of those nervous shocks which are so often fatal to women in her situation. Her face became convulsed, the blood rushed to her head, and complete insensibility followed. For a moment it was feared that she had ceased to live, and consternation reigned around. The Princess de Lamballe fainted, and was borne lifeless through the crowd. The king, with a strength which had acquired additional force from desperation, burst open the lofty windows, which had been closed and secured by bands of paper pasted around them, and admitted a current of fresh air into the room. The gentleman-ushers summarily expelled the inquisitive and indiscreet crowd. The queen was bled in the foot, and as the blood gushed forth, she unclosed her eyes and slowly returned to life. Then a delirium of joy succeeded to the terror of the preceding moment. Tears of thankfulness were shed by all around, and the bedchamber-woman who announced to the noblemen assembled in the *cabinet des nobles* that the queen had recovered consciousness, and had spoken, was publicly embraced by the Austrian ambassador and the Prince de Paix, and literally inundated with their tears.—*Memoirs of the Duchess of Angoulême, the last of the Dauphines.*

HOMŒOPATHIC MEDICAL SOCIETY OF THE STATE OF NEW YORK.

From a notice of the Secretary in another column, it will be seen that the above named society will hold its Annual Meeting on the 8th of next month in the city of Albany. We believe there are from three to four hundred physicians of the Homœopathic school in the State of New York; and we hope to see at least one half of this number at the above meeting.

It should be borne in mind that the *Committee* appointed at the meeting in Syracuse on the subject of a Homœopathic College in this state, will report on the important matter referred to them, which will require the consideration of the society. We assume without any positive knowledge on the subject, that the Committee will favor the establishment of a college in this state, by our school. If the State Society should agree with them, if they report in favor of a college, then it is, we were about to write, absolutely necessary, that every homœopathic physician in the state should zealously cooperate in the measure. This should not be a sectional work. The whole school should feel its interest involved in it, as well as the propagation of a true system of healing.

CASES SHOULD BE RECORDED.

Every physician should feel bound to contribute something for the improvement of the healing art. To this end, a carefully drawn up record of at least one case daily; following it with brief statements from time to time as the treatment goes on, and then the final result. This plan would be of utility to the practitioner himself, as well as a means of collecting valuable facts, which, if laid before the profession through

the medium of our numerous periodicals, would aid in the progress of the art. There is too much reliance on those who rank as the leaders of the profession; because they are not always safe guides. Conventions, Academies, and Societies have not accomplished much in the progress of medicine. Generally, a few minds perform all the mental labor of such associations, while the main body contribute little other than their presence at the meetings, except when elections for officers take place.

We have several times expressed our want of confidence in published cases, we should have qualified the remark by excepting those cases which are transcribed from the original record. We still assert that we suspect the accuracy, unintentional of course, of cases reported from memory, in some instances weeks, months and even years intervening from the time the cases happened and the reports.

THE HOMŒOPATHIC TIMES.

Some of our readers seeing how often we select from the Homœopathic Times, have written to us to know where it is published and what we think of it? We cheerfully answer these questions. The Times is published weekly by Henry Renshaw, 356 Strand, London. We do not know who edits it, for he is so modest that he conceals his name. In form, size, type and paper it is like our own, and of course in our eye it looks well. It is one of the best conducted periodicals of the Homœopathic school. Its doctrines are of the right stamp, clearly stated, and ably defended. And although it would require us to work harder for matter for our own Journal, yet we will submit to this, if some one thousand Homœopathic physicians in this country would subscribe for the Homœopathic Times. Remember, it is published once in each week, and can be mailed in London to any address in America, at a cost of six dollars and fifty cents a year, in advance, except the ordinary newspaper postage in this country.

We make this statement voluntarily; for as we publish our own Journal for the propagation of what we believe a true system of medicine, we scorn the very thought of jealousy, towards a colleague who advocates the same doctrine with becoming zeal, ability and skill. We take leave to direct the attention of our readers to the advertisement of H. Bailliere, 290 Broadway, New York city, who receives subscriptions for the work under notice.

THE MEDICAL GAZETTE HELPING TO PROPAGATE HOMŒOPATHY.

We are almost disposed to say to the editor of the Medical Gazette of this city, here is our ☞ because of the influence he is exerting in favor of Homœopathy. It is well known that opposite causes will produce the same or similar effects. So it is in the instance to which we refer. The Gazette published what it says, are the inconsistencies of Homœopathy, in distinct propositions; in which the editor shows to every *tyro* in that system, that he is utterly ignorant of the subject about which he writes. The Gazette forgets, that he who sincerely desires a knowledge of Homœopathy, would seek for it in the standard works of that school, and not from an openly avowed, reckless and violent enemy. We will pass by some pretty conclusive evidence furnished by the editor himself, that he is a little behind the age in his knowledge of the doctrines of his own school; but we will not meddle with that, but leave it to his own colleagues, who will do him justice.

ALLOPATHIC HOSPITALS IN SPAIN.

The Governor of Madrid has just issued a memorandum of the facts on which he proposes to institute a reform of the hospitals of that capital, in which he draws the following picture of their present condition. He declares that the provisions served out to the patients are of the worst possible description, and dearer than the best, because nobody endeavoured to force the contractors to fulfil their obligations. The scales for the weighing out of the provisions were not equal, so that scandalous facilities were given to fraud. The daily consumption of sheep amounted to forty, the best of which went to the tables of the hospital clerks, and the worst to the patients. Eight hundred pounds of chocolate were consumed every month, not by the patients, because chocolate is not an article of hospital diet, except in rare cases,

but by the *employés*. The kitchens were filthy in the extreme; the number of cooks and kitchen people so great that they hindered each other from working. The mattresses of the patients were half emptied of their wool, the property of the patients; their clothes were taken away without an inventory being made, the bedding unclean, the wards unwashed. There was neither respect nor consideration for suffering humanity, the hygiene of the wards being left to the will and pleasure of hospital clerks and ward lackeys. The dwellings of the insane were horrible to see. The medical case-books slovenly kept, with great intervals between the lines; so that agreeable or alimentitious articles were interpolated in great quantities, but never used by the unhappy patients. The bodies were carried naked to the dead-house in the same cart, without distinction of sex or age, many hours before the term marked down by the law, or dictated by prudence. The hair and the teeth of the dead were converted into objects of commerce. The dispensary full of bad drugs, and, notwithstanding the heap of *employés*, dirty in the extreme. The young students employed as dressers were not under any kind of discipline; were never more than an hour at the utmost in the discharge of their duties; and when required by his Excellency the Governor to perform them, imposed impertinent conditions, and when these were not acceded to resigned their offices, and left the patients without assistance; for which act the Governor put them all into prison, and then deprived them of the permission to enter any public establishment. The clergy belonging to the hospital were by far too numerous, and yet their duties were inadequately fulfilled. The Governor of Madrid has turned out all the inferior servants: he has put the economical part of the establishment into the hands of the Sisterhood of Charity, which contains many women of great merit and abnegation, and he has effected great savings of every kind.—*English Churchman.*

HOMŒOPATHY *via* YOUNG PHYSIC.

By J. Rutherfurd Russell, M.D.

There is a point in the development of a complex science, such as medicine, at which it seems to be overburdened by the multitude of its facts and the insufficiency of its generalisations. When it has reached this stage, and come to a dead lock, it gets no aid from those who have the reputation of being its most successful cultivators. For the very endowments which gave them renown at the time when the science required merely the accumulation of materials, or the critical selection of facts and their lucid arrangement, disqualifies them from the higher task of grasping the subject as a whole, and viewing it by the light of general philosophy, discovering the true center from which it must be re-organised. This great achievement requires more than the highest powers of analytical acumen; it requires the intuition of synthetic genius. And when the voice of the great discoverer, who is destined to renovate his department of knowledge, makes known the central truth, there is none on whose ear it falls more repulsively than on that of the high priest of the special science itself. Great discoveries belong to philosophy rather than to science; and no class of minds is less prepared to receive them than are those who have devoted themselves to the study of scientific details alone.

There is nothing more strange or more lamentable than the total absence of philosophical discernment displayed by those who are now the appointed guardians of medicine. While deploring its present uncertain and unscientific character, they do not seem at all aware of the real cause of its poverty, and propose remedies which would be as useless if got as they are impossible to get. Their inability to perceive what medicine stands in need of, arises from the same cause as their rejection of Homœopathy. If there be anything more striking in Dr. Forbes' memorable article than this, that throughout the whole of it he speaks of theory in medicine as if it were quite an unimportant thing, and as if the whole end of the labors of the medical philosopher were attained if he made himself sure of a sufficient number of facts, it is the constant recurrence of the expression, "philosophic practitioner"—whose philosophy seems to consist in doubting much and in doing nothing. Unsparingly as he condemns Homœopathy, as tending to degrade physic by making its practitioners artisans, the position he would appoint to medicine is singularly opposed to the usual requirements of an art and a science. Science is certain knowledge, giving the power of prediction; art is the application of that knowledge. But his science consists in knowing that we can know nothing, and the art he recommends us to practice is the *dolce far niente!* He speaks of Homœopathy as "an ingenious system of medical doctrine, tolerably complete in its organisation, tolerably comprehensive in its views," and says that "it is as good and rational a theory as most of our medical theories. With the great leader of philosophic practitioners, it seems a very light matter whether a theory be right or wrong. He seems to regard it very much in the light of the shell of an oyster, useful only for containing a nutritious body of facts. It is not surprising that, considering theory to be so very insignificant a matter, he lightly observes,

with an inconsistency which might be culpable in one who took a more serious view of the value of theory, "that we may indeed have sufficient proof to satisfy any reasonable mind that the theory, or doctrines, or principles of Homœopathy are false, although he does not enter into any such proof, but waives it till a more convenient season." He turns away contemptuously from the consideration of the theory, doctrines, or principles, and eagerly asks what the *facts* are worth.

The facts arrest and startle him. He finds that under the guidance of this theory practitioners of medicine restore to health more patients than under the orthodox method, which disdains, as an infringement upon the liberty of the faculty, to acknowledge obedience to any theory whatsoever. This fact of the greater success of Homœopathic treatment, is a hook in the nose of Allopathy, which must in some way or other be extracted. The problem is this: By the most authentic returns, more cures are effected in certain dangerous diseases under Homœopathy than under Allopathy; how, then, is the inference to be prevented that, therefore, Homœopathy is good, and the position to be established that it is radically bad? This is done in the simplest and most ingenious way. Good and bad are relative terms: it is the same thing to say that a man is better than a monkey, as to say that a monkey is worse than a man. It merely requires the transposition of the predicates. If we say Homœopathy is better than Allopathy, we suppose some good in the former —more good, at least, than in Allopathy. But we have only to say Allopathy is worse than Homœopathy to get rid of the dilemma. And this is the position our great antagonist assumes! But it is urged, if Homœopathy be bad, and Allopathy still worse, how do you retain your allegiance to Allopathy? To this he answers: it is bad *in esse*, but good *in posse:* it contains the germs of its entire renovation, and it only requires proper attention to foster the latent rudiments of good into vigorous growth. How comes it, a sceptic might inquire, that you, who have so long held a chief place in the cabinet of medicine—you, her appointed, acknowledged, respected minister—should, till now, never have whispered your belief that the system you have been directing requires a thorough reformation? How is it that this momentous truth has been forced from you by the doings of so insignificant a body as the Homœopathists? Surely in the history of our art, among the triumphs of Homœopathy will be recorded,—it forced Dr. Forbes to give his candid opinion of the state of medicine. It forced him to make confessions during his life, which otherwise he might have reserved for his testament."

But let us follow out the process of Young Physic, and see exactly what it is, and to what it leads: Allopathy and Homœopathy are both bad. In their hospitals many patients die, although more recover. The average mortality in both is much alike; the kind of diseases which kill them is much alike. It is plain that, as there is no great difference in the amount of recoveries under the two systems, they must both derive their efficacy from the same cause. Now, as the methods of treatment are wholly opposite, the benefit must accrue from something beyond or outside of the treatment altogether. The only thing beyond that, common to both, is the natural power of recovery. Here, then, is the real explanation of the cause of the equality in the mortality and recovery in the two sets of hospitals: that those who are able to stand the treatment and the disease get well; and those who are unable die. The practice of medicine (*medendi*, curing) turns out to be nothing. All that the physician can do, is to open his wards and see fair play between nature and death. If the struggle between them end in favor of the latter, he has the satisfaction of confirming his diagnosis; if in favor of the former, of dismissing his patient.

Is it, then, really come to this—that after two thousand years of observing disease, we are no further advanced in its treatment? In what respect is Young Physic superior to Hippocrates? They both advise the same thing: Watch, say they, the progress of disease, but do not interfere. Young Physic, however, remarks, that all who have deviated from this rule have done mischief. All that his greater experience has taught him is, to have greater confidence in the canon of Hippocrates. How long is this new cycle to run? Are we to stand, like sentinel-stars, for other two thousand years, silently watching the course of disease, while ever and anon an eccentric man of action suggests some practical innovation, till, at the expiry of this double millenium, another Forbes arises, and with his critical crow-bar demolishes the systems of these practical men, and dooms his fellow-mortals to endure all the pangs of sickness, with nothing but the consolations of philosophy to alleviate them? In short, what is the germ in Allopathy on which Dr. Forbes rests his hopes? The only positive recommendations we can find are—carefully to watch the progress of disease, to employ all proper hygienic measures, and *to use the numerical method in noting down the effects which have been or may be observed to follow the administration of the medicines.* The first two recommendations, however excellent, can hardly be considered as at all differing from those given by Hippocrates; and as Dr. Forbes

evidently expects much good to accrue from the last, it is to it we shall now direct our sole attention: and if we can establish that the only results of such a method are increasing perplexity, rank error, or total disbelief in the powers of medicine, we must acknowledge the incapacity of Young Physic to be of any use—except in the way of a pioneer.

"*Ars longa, vita brevis, observatio fallax;*" how, then, can any man, in the short term of his life, discover for himself what remedies are certainly useful in the all but infinite variety of diseases he has to treat? Suppose even that he has the advantage of a large hospital, is it possible for him to make experiments in such a way as to arrive at certain knowledge? The Young Physic School, which prides itself upon its philosophy, recommends the use of the numerical method, that, by noting down the result of each medicine in each form of disease, we may at length arrive at positive data to direct us in future cases. Let us inquire what philosophers of established reputation think of this plan.

M. Comte scouts with the same severity as Dr. Forbes all ontological speculations, and founds his system on the ascertainment of positive phenomena alone, leaving the mystics to pry into the cause of these phenomena. He has, moreover, a profound respect for mathematical science, places it as the first and fundamental science in his scheme of human knowledge, and is most anxious to carry the numerical spirit as far into general science as it will legitimately go. He would apply the method of numerical notation to every department of human knowledge where it could be of any avail. Surely, if any philosopher of note is likely to countenance Young Physic's brave attempt to reform medicine by introducing into it the accuracy of arithmetic, it must be M. Auguste Comte. His opinion is given in the following words:—

"Indeed, the spirit of calculation tends in our day to introduce itself into this study (*Physiology*), especially into that part of it relating to medical questions, by a much less direct method, under a much more specious form, and with infinitely more modest pretensions. I wish to speak of that pretended application of it which is called the statistics of medicine, from which many savans [*e. g., Young Physic*] expect wonders, and which, from its very nature, can lead only to profound and direct degradation of the medical art (reduced by it to a blind enumeration). Such a method, if we may be allowed to call it by the name of method at all, cannot in reality, be anything else than absolute empiricism, disguised under the frivolous garb of mathematics. Pushed to its extreme logical consequences, it will tend to make all rational medication radically disappear from medicine, by conducting the practitioner to make chance trials of certain therapeutic measures, for the purpose of noting down with minute precision the numerical results of their application. It is evident, on principle, that the continual variations to which all organism is subjected, are necessarily even more pronounced in a pathological, than in a normal state, as a result of which the cases must be even less exactly similar, whence results the manifest impossibility of making a judicious comparison between two curative methods derived from data furnished by statistical tables alone, independent of some sound medical theory. No doubt some direct experimentation, restrained under proper limits, might be of great importance to medicine as well as to physiology, but it is precisely under the strict condition that it shall never be simply empirical, bnt that it shall always attach itself either in its institution or in its interpretation to an entire system of corresponding positive doctrines. (*A l'ensemble systématique des doctrines positives correspondantes*). Notwithstanding the imposing aspect of the forms of exactness, it would be difficult to conceive of an opinion in therapeutics more superficial and more uncertain than that which rests solely on the easy computation of fatal and favorable cases, to say nothing of the pernicious practical consequences of such a manner of proceeding, where one could not beforehand exclude any kind of attempt.

"It is really deplorable that geometricians have sometimes honored with some kind of encouragement such a profoundly irrational aberration, by making vain and puerile efforts to determine, by their illusory theory of chances, the number of cases sufficient to make these statistical results legitimate." —Cours de Philosophie positive, par M. Auguste Comte. Tome 3me pp. 418, 420.

We must again remind Young Physic that M. Comte is one of the first of living mathematicians, and one who, more than any other philosopher, insists upon the prolongation of the mathematical method as far forward into the more special and complex sciences as it can be made to go.

(*To be continued.*)

Some of our zealous and kind-hearted colleagues virtually tell us, that they know much better than we do, how to carry on the work of reform in medicine; this may be so, but the difficulty is to convince us of that fact.